

## Contemporary Advances in Physics, XXX—The Theory of Magnetism \*

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The topic of this article is the explanation of magnetism as ordinarily observed—to wit, the magnetization of pieces of matter of ordinary dimensions—by ascribing magnetic moment to the individual molecules, atoms, and electrons of which matter is composed. For *paramagnetic* bodies it is postulated that the individual atoms are magnets of which the orientation, but not the strength, is altered in the presence of a magnetic field; the theory is so successful as to make it possible to calculate, from magnetization-curves, values for the magnetic moments of these atoms which agree admirably with those deduced from spectroscopic theory and from experiments of other types. For *ferromagnetic* bodies the same postulate is made, but it is necessary in addition to recognize the existence of huge interatomic forces of which very little is known, so that a large proportion of the science of ferromagnetism still lies beyond the scope of atomic theory. For *diamagnetic* bodies the phenomena are interpreted in a simple and effective manner, as an immediate corollary of the well-known structure of the atom.

**M**AGNETISM is a quality which we attribute to the atom. We affirm that iron, nickel, gadolinium, gaseous oxygen, and in fact all substances, are magnetic because there is magnetism in their atoms. Indeed we go even deeper, and affirm that the individual electrons and the nuclei within the atoms are magnetic. Nevertheless, the atomic theory of magnetism is a really valuable theory. Perhaps that “nevertheless” sounds out of place; but I assure you that without it there would be a trace of paradox in the statement, which perhaps our grandfathers would have been quicker at discerning than are we. Let me explain my meaning by referring to the atomic theory, or as it is usually called the kinetic theory, of gases. Those who designed this theory succeeded in explaining the pressure, the temperature, and the viscosity of gases, without attributing a single one of those qualities to the atoms. To the atoms they assigned the properties of momentum and velocity and kinetic energy; those other qualities which I just named were then interpreted in terms of these,—they were interpreted as what we call *statistical* properties of the great multitude of atoms which constitutes a gas. This was a real explanation of pressure and viscosity and temperature, in the fullest sense of the word “explanation”—or anyhow, in the fullest sense of that word which is customary in physics. But along with these properties of pressure and viscosity

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and temperature, a gas also possesses weight. The builders of the kinetic theory simply said that the weight is a property of the individual atoms, and that the weight of the gas is the sum of the weights of its atoms. Now evidently this was not an explanation of weight at all. Indeed, by assigning weight to the individual atom, the builders of the theory had foregone all attempts at an explanation. A property which you assign to the atom is a property which you refuse to try to explain in terms of the atom—or so at least it always seemed to our forefathers. To assign a quality to an atom used to be taken as a confession of incompetence to explain that quality. I can of course make this clear by proceeding to absurd extremes. If I say that an orange consists of soft yellow juicy atoms, or that a marshmallow is made of sweet white sticky atoms, or that a piece of iron is made of hard black shiny conductive atoms, you recognize at once that those are not serious atomic theories: they are just futile and somewhat ridiculous statements. If I claim to explain the weight of a piece of iron by saying that it is the sum of the weights of the atoms, I am making a claim which unfortunately may not sound ridiculous, but is really just as futile—unless it acquires value by being linked with some other assertion. But when I say that the magnetism of a piece of iron is due to the magnetism of its atoms and its electrons, the statement is by no means a futile one; it is significant and important. For this there are two main reasons or rather groups of reasons, which I will indicate by the words *orientation* and *atomic structure*. (In addition there are remarkable experiments on jets of atoms whereby their magnetic moments are measured directly, but these I reserve for another occasion.)

First a few words about atomic structure. It is a fact of experience—the experience of one hundred and fifteen years—that a current running around a loop of wire is the equivalent of a magnet. If now somebody asserts first that a piece of iron is magnetic because its atoms are magnets, and then goes right ahead and asserts that the atoms are magnets because they have perpetual currents running around inside them—well, the combination of these two statements is not necessarily futile or trivial. At the very least, it is a sensible attempt to reduce the two kinds of magnetism apparently existing in the world to a single kind, that which is due to moving electricity. This was Ampere's idea a hundred years ago. Now if in addition there is independent evidence that the atom comprises mobile electrical particles, then this idea of Ampere's becomes the assertion that those particles inside the atom are actually revolving. It is well known that modern physics is full of such evidence, of evidence that atoms contain very mobile electrons; and some of my readers may recall that thirty years or so ago there

were an atom-model with stationary electrons and an atom-model with revolving electrons, which were in competition with each other, and the latter of which has by now driven the former utterly out of the field. Remember now, that the atom-model with the revolving electrons triumphed over the other one not primarily because of its magnetic quality, but because of the theory of spectra which Bohr and others were able to derive from it. Revolving electrons in atoms were first of all proved to be responsible for spectra, and then it was noticed that they are capable of causing magnetism. Therefore when the physicist says that magnetism is a quality of atoms, he is not making a confession of incompetence, but an inference from a highly-developed and successful theory of spectra; and this makes all the difference in the world to the value of the statement. Indeed the situation is even better than I have intimated; for there are dozens of cases in which first an atom-model or a molecule-model has been constructed expressly to explain the spectrum of the substance in question—then, the magnetic moment of its system of revolving electrons has been computed—then, the magnetic moment of the atom or the molecule has been measured—and the two have agreed! This is really an understatement, which needs to be broadened so as to include the cases in which the spin of the electron plays a part; but I pass them over, intending to defer the broadening to the latter part of the article, which is to be devoted to these matters of atomic structure. For the moment, let me make just one more allusion to them, a very important one. Electrons revolving in orbits around a nucleus obviously possess angular momentum. Therefore, if an atom has a magnetic moment due to revolving electrons, it has an angular momentum also. This again is an understatement, for it contains a restriction which can be removed in view of the broadening which is later to be made. It appears to be a general rule that *in the atom, magnetic moment and angular momentum always go together*. A magnetic atom is a gyroscope—necessarily and automatically. This is a fundamental principle, and from it flow some strange and striking consequences. Everyone who has worked or played with the classic gyroscope of our laboratories knows that it has quaint and tricky idiosyncrasies. Well! the atom has them too; but it has others in addition. Angular momentum, on the atomic scale, is subject to peculiar laws of quantum mechanics; and the atomic magnet-gyroscope behaves in extraordinary ways, of which our laboratory gyroscopes give not the faintest intimation.

To summarize my introduction then: the first step in the theory of magnetism consists in referring it to the individual atom. This sounds like a confession of defeat, but it is nothing of the sort; it is a claim of

victory. Our theory of spectra requires that atoms, or some of them at any rate, should be magnets, and they *are* magnets. Moreover it fixes the magnetic moments which certain atoms ought to have, and so far as our experiments go, the atoms *do* have these moments. Moreover it imposes angular momentum on these atoms, and fantastic as the consequences are, experience bears them out. Logically, then, I should begin the main part of my talk by showing how the magnetic moments and the angular momenta of atoms and of molecules are calculated from their spectra by atomic theory. This, however, would by itself require several lectures, and very difficult ones at that.\* I must therefore simply ask you to believe that the magnetic moments of atoms are inferences from fundamental theory, not mere *ad hoc* assumptions; and now I will explain what I had in mind when I wrote down the word *orientation* to designate one of the topics of this article.

It is one of the best-known facts of physics that the magnetization of a substance is not fixed and constant, but increases with the strength of the magnetic field which is acting on the substance. By the way, before going any further I must definitely exclude the so-called "diamagnetic" substances. That exclusion being made, we do *not* assume that the magnetic moment of the individual atom increases similarly with the field strength. People did not make that assumption, even in the days before the fundamental theory was developed. Had they done so, it would have been just as silly as saying that a marshmallow is made of soft white sticky atoms, and calling *that* an atomic theory. They supposed, and we suppose, that the moments of the individual atoms remain practically the same whatever the field strength; what changes is the average *inclination* of these moments to the field. The atomic moments are vector quantities pointing in various directions, different from one atom to the next. The magnetization of the substance as a whole is the resultant of all these myriads of tiny vectors pointing in their various directions. If they all pointed the same way the substance would be completely and perfectly magnetized, with a moment equal to the total number of the atoms multiplied into the moment of any one. This state of *saturation* is not, however, to be attained, not even to be approached without a rare and felicitous concurrence of a favorable substance, a very low temperature and a very strong field. Much easier of attainment is the opposite extreme, when the vectors are pointing all ways at random and the magnetization is zero. This happens with nearly all substances when there is no field applied, and it seems quite natural. But when even the smallest field

\* This subject was partially treated in "Contemporary Advances in Physics, XXIX . . .," April 1935 *Bell Sys. Tech. Jour.*

strength is applied to such a substance, you might expect all the little magnets to turn right around and point straight up the field-direction, achieving saturation in an instant. Well, it is certain that saturation is not achieved; but still there is some degree of magnetization, as though the little magnets all started to turn around and were stopped before they got very far. What is it that might stop them? If you look at the books of twenty or twenty-five years, you will find an answer: they are stopped by the collisions which these atoms make with one another. This is the classical idea, which is generally thought to be well verified by experiment. But let us look into the matter a little more closely.

For simplicity let us imagine a gas—preferably, unit volume of the gas—composed of  $N$  identical atoms, each with a magnetic moment  $\mu$ , and exposed to an applied field  $H$ . Visualize some particular atom, of which the career is an endless alternation between free flights and sudden impacts. All the time the magnetic moment of the atom, the little vector of which I have been speaking, is subject to a torque arising from the field. The classical idea is, that throughout every free flight that torque is steadily bringing the vector more and more nearly into alignment with the field, but usually not having time enough to succeed, because at every collision the vector is suddenly and violently re-directed in a perfectly arbitrary way. Gradual approach to alignment during the free flights, violent dis-alignment at the collisions, and the magnetization of the substance indicating how far the alignment progresses, on the average, before the dis-alignment stops it—this is the classical picture. It all seems beautifully obvious, and yet is it now believed to be entirely false!

The trouble lies in the fact that the atom is a gyroscope. You recall that it is one of the oddities of the gyroscope that when you apply a torque to it, it starts off at right angles to the direction in which you expect it to go. Now here is our atom just leaving the scene of a collision with its magnetic moment making, say, an angle  $\phi$  with the field-direction. As it flies away the torque is steadily trying to reduce the value of  $\phi$ , but instead of obeying, the atom just keeps on blandly precessing about the field-direction, the value of  $\phi$  remaining obstinately the same. The unbreakable link between magnetic moment and angular momentum has neatly killed the supposition that the field magnetizes the gas because it aligns the atoms, or partially aligns them, during their intervals of unimpeded flight. The free flights are just the periods when nothing whatever happens in the way of alignment. Much labor has been expended in the hope of finding some way out of this impasse, but none has been revealed except that of supposing that

whatever is the mechanism whereby the field aligns the atoms, it is a mechanism which operates during the impacts and not between them. Partial alignment *at* the collisions, no change in the situation during the free flights—this amounts pretty nearly to standing the classical theory on its head!

Nevertheless the mathematics of the classical theory remains entirely unchanged. This is because the mathematics merely expresses the assumption that the field has managed to find *some* way of partially aligning the atoms, and does not concern itself in the least with what that way may be. This sounds rather vague, so let me remind you just what the assumption is. Suppose to begin with that the vectors of the atoms are capable of only two orientations in the applied field: one parallel, the other anti-parallel to the field-direction. To transfer an atom from the one orientation to the other, we must do work against the torque of the field (or receive work from the torque of the field) amounting to  $2\mu H$ . We have, therefore, two classes of atoms, differing in energy by  $2\mu H$ . Let  $N_1$  and  $N_2$  stand for the numbers in these classes at some particular instant. Now the classical theory, as I have been calling it, is strictly no more than the assumption that the ratio of  $N_1$  and  $N_2$  is given by Boltzmann's theorem:

$$N_1/N_2 = \exp(-2\mu H/kT) \quad (1)$$

and the essence of this assumption, I take it, is that the atoms are able to change their orientation so as to pass from either class to the other, and that they employ this facility of free passage to get themselves into thermodynamic equilibrium at the temperature  $T$  of the gas. This has been the assumption ever since Langevin founded the theory, and it still is the assumption, even though we may no longer enjoy that pretty picture of the mechanism of the change of orientation which once we accepted, and have no other to replace it.

I can readily write down the complete theory of this case. We introduce the two additional equations,

$$N_1 + N_2 = N, \quad I = (N_1 - N_2)\mu, \quad (2, 3)$$

of which the first says that all the atoms are in either the parallel or the anti-parallel class, and the second that the magnetization  $I$  of the unit volume of gas is the resultant of the vectors of all its atoms. Now we eliminate  $N_1$  and  $N_2$  between the three equations, and swiftly arrive at the result:

$$I = N\mu \tanh(\mu H/kT), \quad (4)$$

which is the equation of a curve starting obliquely off to the right from

the origin, and bending over to approach an asymptote which is a horizontal line of the ordinate  $N\mu$ .

At this point a strictly classical physicist would certainly grin or sneer, because he would say to himself: "The speaker started out by assuming for simplicity that the atoms can point in only two directions, and now he has gone on to his conclusions without remembering the obvious fact that an atom may point in any direction whatever!" Well, of course Langevin did make allowance for that supposedly obvious fact; it complicates the affair to some extent, but not seriously, and leads to a very similar curve for  $I$  versus  $\mu H/kT$ . Quantum mechanics, however, flatly denies that it is a fact. I mentioned above that the atomic gyroscope has some paradoxical properties of its own, in addition to those which it shares with the laboratory gyroscope. Here is one of them. *The atomic magnet is supposed to be able to set itself, not at any angle whatever with respect to the applied magnetic field, but only at one or another of a small number of definite discrete angles.* This is because of its angular momentum: it is primarily the angular momentum which is constrained to this very singular behavior, and which the magnetic moment is automatically obliged to follow because they are so closely linked together. If I am asked why the angular momentum should behave like this, I can only reply that according to what I am told, if one is sufficiently penetrated with the spirit of quantum mechanics it seems self-evident, and if one is not sufficiently penetrated with that spirit there is nothing which can be done to help. Notice anyhow that it is compatible with the statement that when the atom is freely flying along, the field just keeps it precessing about the field-direction, instead of gradually aligning it; and there is ground for being thankful that this derivation, and certain others, are somewhat simplified by it. It may be asked, how many different inclinations are permitted to the atom? This depends upon the angular momentum of the atom, and we can tell it from the spectrum. There are certain elements and certain compounds for which the case is just as simple as I have described it; just two permitted orientations, the parallel and the anti-parallel, and no more. There are others for which the permitted inclinations are three in number, others for which there are four, five, and other integers up to fifteen or twenty. All these yield curves of  $I$  versus  $\mu H/kT$  having the same general traits, but differing in the rate at which they approach the asymptote. I will refer to all such curves as "Langevin curves," although the only one which Langevin proposed was the classical curve corresponding to the case in which all orientations are permitted (or, as we may say, there are infinitely many permitted orientations).

You may now be expecting me to say that there are many gases, and possibly other substances as well, for which experimental curves have been obtained that are comparable with these. I am obliged to disappoint you. You can readily see that in order to get over onto the "curvy" part of these curves, one must work in experimental conditions in which the argument  $\mu H/kT$  is greater than, or anyhow not very much less than, unity. One thinks, of course, of using the highest accessible field strengths  $H$  so as to enhance the numerator of that fraction. This, however, is not sufficient, for it turns out that  $\mu$  (the magnetic moment of an atom or a molecule) is so very small that one is obliged to diminish the denominator also by going to the lowest attainable temperatures. All the experimental curves of this character have been obtained at temperatures lower than  $15^\circ$  absolute, some at temperatures between  $1^\circ$  and  $2^\circ$  absolute. This excludes all the gases. Moreover, it has been necessary thus far to choose the atoms with the largest magnetic moments, and these turn out to be, quaintly and inconveniently enough, the atoms of the rare-earth elements. Probably the best of the experimental curves (Fig. 1) relates to a substance which most people never have heard of, in this or any other connection: it is gadolinium sulphate. There are about a score of such curves ob-

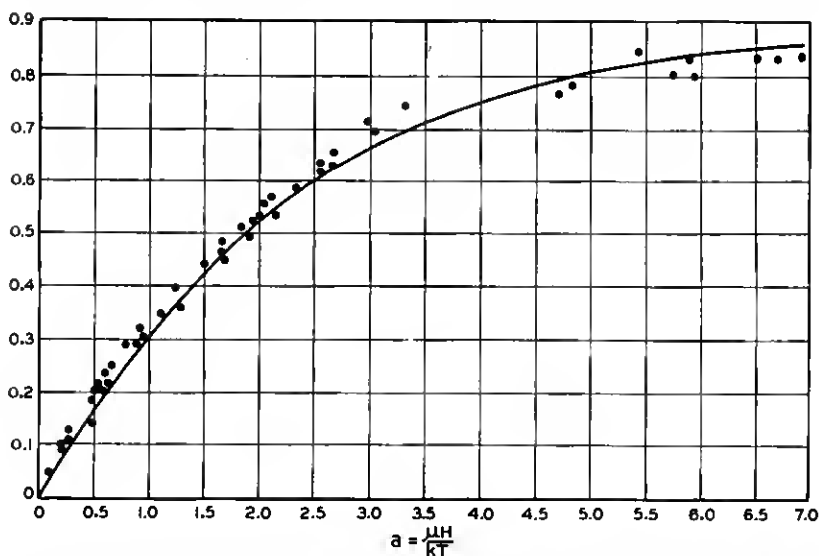


Fig. 1—Magnetization of a paramagnetic salt  $[\text{Gd}_2(\text{SO}_4)_3 + 8\text{H}_2\text{O}]$  as function of the parameter  $a$ ; the ordinate is  $I$  referred to its saturation-value (deduced by extrapolation) as unity. Data from Onnes and Woltjer. The curve is the "classical" Langevin curve (number of permitted orientations,  $n = \infty$ ) which is hardly distinguishable from the quantum-theory curve for this particular case ( $n = 15$ ).



tained with minerals and glasses containing these elements, and most of them agree well with one or another of the theoretical curves; in which connection there is an interesting detail, which I will bring up at the end of the article. One would scarcely expect a theory worked out for gases to apply so well to solids, and as a matter of fact it is a peculiarity of the rare-earth atoms that even when incorporated in a compound or a solid they behave in several ways more like the atoms of a gas, than happens with any other elements.

Pray do not think, however, that all this time I have been talking about a theory which has no application excepting to the rarest of all elements under the rarest of all temperatures. Its applications are a good deal wider than that. True it is that with gases universally, and with other substances ordinarily, we cannot get data along the curvy parts of the curves; but we can make measurements along the sensibly-linear parts near the origin. This amounts to saying that we can determine the slope of the curve at the origin. Now of course it sounds ridiculous to speak of confirming a theoretical curve by measuring its tangent at one point. In this case, however, it is not altogether ridiculous. Usually the experiments are made by varying  $H$  and measuring  $I$  while the temperature is kept constant. Suppose this is done for several different temperatures, and suppose the results are plotted by using  $H$  instead of  $\mu H/kT$  for the abscissa. Then the theory supplies us with different curves for the different temperatures, all having the same general aspect, but different slopes at the origin. I will denote these slopes for the time being by  $\tan \theta_0$ . The theory, then, requires that  $\tan \theta_0$  should be proportional to  $1/T$ ; and for gases, this is found to be the case. Of course this is not such good evidence for the theory as would be a complete following-up of the curve nearly all the way to the asymptote; but it is pretty good by itself, and for further evidence we can invoke those experimental curves for gadolinium sulphate and other solids of which I just spoke.

If now we let ourselves be convinced by this evidence, a valuable conclusion follows. From the slopes of these curves at the origin, the value of  $\mu$  can be deduced. Let us go back to the curve of  $I$  versus  $\mu H/kT$  or  $a$ , which is the epitome of all the rest. We write:

$$dI/da = N\mu(1 - \tanh^2 a), \quad (5)$$

$$(dI/dH)_{T=\text{const.}} = (dI/da)(da/dH) = (1 - \tanh^2 a)N\mu \cdot (\mu/kT). \quad (6)$$

Since measurements are actually made at a fixed temperature and refer to the slope of the curve near zero field strength, we evaluate this derivative for  $a = 0$ , and we get:

$$\tan \theta_0 = (dI/dH)_{H=0} = N\mu^2/kT, \quad (7)$$

and thus from the measurement of  $\theta_0$  at any temperature we derive the magnetic moment of the individual atom or molecule of the gas. This formula ought to give the right order of magnitude for  $\mu$  in any case. Whether or not it will give exactly the right value, will depend on the validity of one of the assumptions, which I now recall. This particular formula is for the case in which the atoms have only two permitted orientations in the field, the parallel one and the anti-parallel one. Had we supposed that every inclination is a permissible one, we should have arrived at  $(1/3)N\mu^2/kT$ . Had we supposed a number of permitted inclinations greater than two and less than infinity, we should have arrived at some intermediate value. So, I now write as the general formula,

$$\text{volume-susceptibility } \chi = bN\mu^2/kT, \quad b = 1 \text{ to } 1/3, \quad (8)$$

having placed on the left the name and the symbol by which is generally known what I have been denoting by  $\tan \theta_0$ , and on the right a factor  $b$  of which the value will depend on the number—I will call it  $n$ —of permitted orientations, but will fortunately never be outside of the narrow range between unity and 0.33.

Thus a rough estimate of an atomic moment may be made without knowing the number of the permitted orientations. Very many such estimates have been made, and they always give values of  $\mu$  quite compatible with what we know in general about the structures of the atoms. If we want to make an estimate truly accurate enough to serve as a stringent test of theory, then we must take from the spectrum of the atom, not only the spectroscopic value of magnetic moment with which we are going to make the comparison, but also the angular momentum of the atom which is what determines the number of orientations. This causes us no extra trouble, for if we understand the spectrum well enough to get the one we also understand it well enough to get the other. Now when we look into the literature to see how many such comparisons have been made, we suffer again a disappointment. It turns out that the noble gases and most other convenient gases exhibit the magnetic moment *zero*. This is of course no fortuitous bit of ill luck; it is the same thing, to wit a certain stable interlocking of the various electronic orbits and rotations in the atom, which leads on the one hand to a zero magnetic moment and on the other hand to a relative smallness of the forces which make for chemical combination and for condensation. Anyhow it is an inconvenience; but luckily there are two convenient gases, oxygen and nitric oxide— $O_2$  and  $NO$ —which do have magnetic moments different from zero; and the test of the theory is in these cases most satisfactory. The agreements be-

tween the magnetic moments calculated from magnetic data after this fashion, and those derived from the spectra, are accurate within an experimental uncertainty of a few promille. I think that these are among the most impressive results in the whole structure of modern physics. Then in addition the rare-earth elements help us out again, owing to the peculiarity which their atoms have of behaving, even when they are incorporated into solid compounds, as though they were the atoms of a gas. They have supplied us with a number of beautiful agreements of this same character.

Now as a transition to the next part of this paper, I must acquaint you with another fact which belongs to this last part. I have more or less been allowing you to suppose that with solids as with gases, the susceptibility is generally proportional to  $1/T$ . Actually it is much more common, among solids, to find a law of the type,

$$\chi = \text{const.}/(T - \theta), \quad (9)$$

where  $\theta$  stands for a constant differing from one substance to another. This constant is evidently of the dimensions of temperature; it is a sort of "critical" temperature, known as the *paramagnetic Curie point*; the formula usually holds for a broad range of values of  $T$  above and not too close to  $\theta$ . (There are plenty of cases where even this formula will not fit, but we will not concern ourselves with them.) You see that this might be taken as meaning, that for temperatures greater than  $\theta$  the substance is more strongly magnetized by any particular field strength than, by our previous theory, we should expect it to be. It might even be taken as suggesting, that in addition to the applied field which we produce ourselves by a horseshoe magnet or something of the kind, there is an extra field arising within the substance itself, which helps along with the magnetization. Now this is just the suggestion which physicists have accepted. Of course it is necessary to make some specific assumption about this extra or internal field, in order to arrive at the empirical law which I just wrote down. The required assumption turns out to be simple and gratifying. It is necessary and sufficient to assume that inside the magnetized substance, there arises an extra field which is proportional to the magnetization  $I$  itself. Hitherto we have been supposing that the torque acting upon an atomic magnet is directly and entirely due to the applied field  $H$ , and we have been led to the law that  $\chi$  varies inversely as  $T$ . Now we are going to suppose that the torque is due to a field  $(H + AI)$ ; and this will lead us, by way of the equation

$$I = N\mu \tanh (H + AI)/kT \quad (10)$$

to the law that  $\chi$  varies inversely as  $(T - \theta)$ . The constant  $A$  is one which we adjust so as to get the empirical value of the constant  $\theta$ . It is pleasant to be able to say that this constant  $A$ —that is to say, the hypothetical extra field—does not meddle at all with the multiplying factor: the theory allows us to write:

$$\chi = \frac{bN\mu^2}{k(T - \theta)} \quad \begin{array}{l} \theta \text{ depending on } A \\ b \text{ depending on } n \end{array} \quad (11)$$

and (if it is the right theory, of course!) we can go on estimating atomic moments for substances of this category just as well as we can for the substances for which  $\theta$  is zero. Most published values of  $\mu$  correspond to such cases.

I am going to say very little about the extra field, or "Weiss field" as it is often called, because it is still one of the mysteries of physics. One realizes readily, of course, that if all the little atomic magnets turn themselves partially or totally into alignment, each one of them experiences a magnetic torque which is due to all the rest. It may be shown that this is proportional to the magnetization  $I$ , which looks very promising indeed; but alas, when it is calculated its magnitude turns out to be thousands of times too small. People used to say that  $AI$  must be a field of non-magnetic origin, which is just another way of saying the same thing. At present it is commonly believed that the force in question is what is called an "exchange" force, that is to say, an electrostatic force among electrons, of which the *modus operandi* can be discovered only by quantum mechanics. I am told that this quantum-mechanical theory has not yet been persuaded to deliver a really satisfactory result; but probably we shall be obliged to accept it in default of any other.

Now I call your attention to the fact that if the temperature should be made equal to or lower than  $\theta$ , this last equation would predict something very wild and strange: an infinite, or a negative, susceptibility. This is a curious situation, and there are several cases in which we can appeal to experiment to resolve it. Take the elementary metal *nickel*, for example; if one measures the susceptibility over the range between 400° and 900° C. one gets a gorgeous curve of just this character, for which the value of  $\theta$  is around 370°; now if one investigates nickel at temperatures below 370°, say around room-temperature, one learns that it is *ferromagnetic*. The same holds true for iron, for cobalt, for a diversity of alloys, except that  $\theta$  varies from one case to another.\*

\* There are however cases in which the substance does not display the distinguishing marks of ferromagnetism (notably remanence) when  $T < \theta$ ; and incidentally there are cases in which  $\theta$  is negative; all of these are knotty problems for theory.

I will just refer to one additional case, because it is of very recent discovery and relates to that rare element which is so helpful in magnetics and seems to be so useless for anything else: I mean *gadolinium*. Metallic gadolinium has a value of  $\theta$  amounting to about  $300^\circ$  absolute. Well, last spring Trombe at Strasbourg investigated this metal at low temperatures and found that it, too, is ferromagnetic, even more so than iron itself. Incidentally most of the rare-earth elements have not yet been prepared in pure metallic form, and it looks as though we might almost count on turning up some more cases of this kind. All this brings me to the question of *ferromagnetism*.

I do not suppose that any of my readers thinks that it is ferromagnetism of which I have thus far been speaking, but for the sake of completeness I will give the name: up to this point we have been considering *paramagnetic* bodies, and explaining their behavior by the orientations of atoms in fields. Now we turn to the properties of iron, cobalt, nickel, various alloys and compounds of these, various alloys containing manganese, and gadolinium: the *ferromagnetic* substances.

The most confusing thing about ferromagnetism—at least if my own experience as a student is any guide—the most confusing thing is, that the *I*-vs-*H* curve of a ferromagnetic substance reminds one of the sort of thing that the Langevin theory is meant to explain, and yet it is not that sort of thing at all. One looks at the Langevin curve with its approach to saturation, and then one thinks of the curve for iron with *its* approach to saturation, and one cannot help but think that the two must correspond to each other except for minor and trivial details. Well, they do *not*. They differ not alone in trivial details, but in every possible way, excepting the solitary common feature of the approach to a horizontal asymptote.

It is really impossible to put this statement too strongly. The Langevin curve and the iron curve differ in shape, as any sketch (cf. Figs. 1 and 2) will show. They differ utterly in scale. If I were to start to put a Langevin curve on the same plot where an iron curve appears with suitable detail, not only would it be sensibly linear for thousands and thousands of miles, but it would not even rise appreciably off the axis for hundreds of miles. Conversely if I had tried to put the curve for a ferromagnetic body upon the same graph as the Langevin curve, the former would have consisted only of the axis of ordinates plus the horizontal asymptote. Finally, the temperature relations are all wrong. I told you that in the Langevin curve the slope near the origin varies inversely as temperature, and I left you to infer that the ordinate at saturation is independent of temperature. In the curve for iron, the slope near the origin goes up with the temperature, and the

ordinate at saturation goes down as the temperature goes up. And yet, we interpret ferromagnetism by what is essentially an atomic theory: that is to say, we suppose that any piece of iron is an aggregate of little magnets each having a constant magnetic moment (so long as the temperature is kept constant) and that magnetization of iron consists in aligning these magnets.

I think it instructive to refer to these little magnets by the name of "atom," with some distinctive prefix; so, for a few minutes, I will call them "super-atoms," though this is not the customary name. When a piece of iron is unmagnetized or demagnetized, the super-atoms are

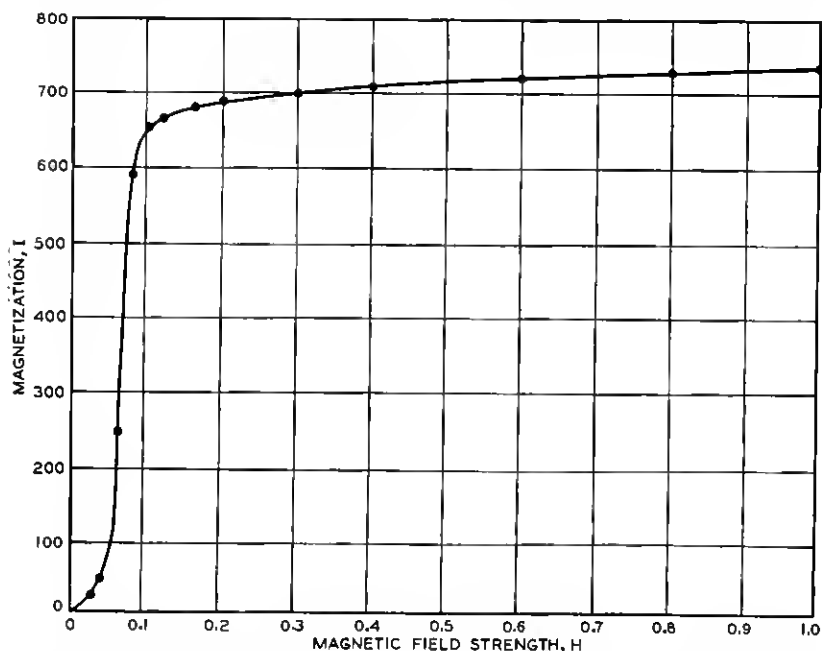


Fig. 2—Magnetization of a ferromagnetic material (81 permalloy, annealed two minutes at  $1000^{\circ}\text{C}.$ ); the ordinate is  $I$ . Data by P. P. Cioffi.

pointing in all directions at random, just like the individual atoms of a paramagnetic gas which is unmagnetized. When a magnetic field is applied to the unmagnetized iron, the super-atoms get more or less aligned with one another. If the field is strong enough they are perfectly aligned, and there exists what is usually called "saturation" of the iron. Now it is worse than useless to remember about Boltzmann's theorem, or impacts, or free flights between impacts, for all those concepts have no relevance. We have to look at the phenomena, and see what they require.

We see at once that the super-atoms must be very easy to align, because saturation comes so quickly, with so relatively small a field strength. We learn also that when they are aligned, they are not exposed to the incessant urge to utter dis-alignment which afflicts the atoms of a paramagnetic substance, for iron continues to be magnetized when the field is withdrawn; not fully magnetized, as a rule, but considerably so. Heretofore I have been talking of substances, in which the atoms have a natural state of perfect dis-alignment or random orientation; a moderate field can derange it only a little, and the atoms return to it instantly and invincibly as soon as the field is cancelled. Now I am talking of substances in which the super-atoms seem to have no single natural state at all; a moderate field aligns them with ease, and when it is removed they like to linger in their alignment. The phenomena become clearer when we experiment not with ordinary iron, which is a chaotic mass of tiny crystals, but with a single large crystal. It turns out then that the super-atoms have a mighty preference for pointing along the cubic axes as distinguished from all the other directions; but as between these three cubic axes, and as between the two opposite senses along each of the three, they seem to be well satisfied with any. Suppose for definiteness that I have a cubic crystal of iron with one of its axes vertical, another in the meridian and the third, of course, pointing east and west. Then if the crystal is unmagnetized, one sixth of the super-atoms may be pointing east and one sixth west, one sixth pointing north and one sixth south, one sixth pointing up and one sixth down. (I do not say that this is necessarily the case, but it may be.) Now if I apply to the crystal a moderate magnetic field pointing north, the one sixth of the super-atoms which were already pointing north will not be affected, but all the other five sixths will flop right over and imitate them. It is amazing how small a field will suffice to do this: 100 oersteds for a good single crystal, whereas 100,000 oersteds, as I suggested, are not enough to bring the ordinary paramagnetic substance at room-temperature anywhere near to saturation. If next I cancel the field, the five sixths of the super-atoms which came over to the northward orientation will not be irresistibly urged to hasten back to their previous habit: indeed if I manage to avoid mechanical shocks and jarrings, most of them may linger indefinitely, still pointing in the direction to which the vanished field once tempted them. Some readers may notice an odd resemblance between this and the earlier case, in that the super-atoms have a finite number of discrete orientations, just as the atoms do. This resemblance is, however, so superficial and (probably) misleading, that I might not even mention it if I could be sure that it had not been

observed. To state two points of difference among many: the "permitted" directions for the super-atoms depend upon the crystal structure, those for the atoms depend upon the field-direction and the angular momentum of the atom; and if one applies a field to a single crystal in any direction oblique to all of the cubic axes, the super-atoms will consent to point in that direction, provided the field strength is rather high.

Now I must explain what these super-atoms are, since our understanding of them is one of the most satisfactory features in our, as a whole very imperfect, theory of ferromagnetism. They are groups—commonly called *domains*—of adjacent individual atoms; the member-atoms of each domain are behaving like the atoms of a paramagnetic solid. A diagram of a ferromagnetic solid might be drawn as an assemblage of large arrows, each representing the magnetic moment of a single domain; then, around and beside each of these large arrows might be drawn a lot of small arrows representing the magnetic moments of the individual atoms constituting the group; the big arrow would be the resultant of all the little ones. It would not be practicable to do this accurately, for there would have to be millions, or millions of millions, of little arrows to each of the big ones; but even a few suffice to show the idea. It may, however, be recalled that I have lately said that the atoms of a paramagnetic body have an irresistible urge to be in a state of random orientation whenever there is no applied field acting upon them. The resultant of all the little arrows of a domain should then be zero. How can it have a magnitude which is not merely different from zero, but (on the scale customary for such things) very considerable, and independent of the field strength which is applied to the iron?

The answer to this question is given, and very well given, by that extra field or "Weiss field" within the group, which I first mentioned in connection with the constant  $\theta$  which paramagnetic solids exhibit. It will be remembered how this constant is explained by assuming that the torque, which acts on any one of the atomic magnets, is due not entirely to the applied field  $HI$  but to the resultant of that and an extra field  $AI$  which is proportional to the magnetization  $I$  of the body. We have already had the equation (10) which links  $I$  and  $H$  when this extra field is present. Now striking  $HI$  altogether out of that equation, we arrive at this one:

$$I = N\mu \tanh(\mu AI/kT) \quad (12)$$

which refers to a situation in which there is no applied field at all. This may be regarded as an equation for  $I$ , fixing the value or values



of  $I$  which can exist in this situation. Now everything which I have said so far encourages the reader to suppose that the only possible value of  $I$  in the situation is zero; and as a matter of fact, zero is always a solution of this equation. But suppose that there should be another solution, different from zero. The equation would then assert, that if somehow that value of magnetization should arise in the substance, then the extra field would also arise, and in just the right magnitude to maintain that magnetization perpetually, without any aid in the form of a field applied from the outside.

Well, the equation is not exactly easy to solve for  $I$ , but it can be mastered—most conveniently by a graphical way—and the striking result is reached, that if  $T$  is greater than  $\theta$  there is no other solution than  $I = 0$ , but if  $T$  is less than  $\theta$  there is a second solution. I will denote this other by  $I_w$ . Consider, then, the situation when there is no applied field: if the temperature is higher than  $\theta$ , I repeat what I have been saying all along, that random orientation of the atomic magnets is inevitable; but when the temperature is lower than  $\theta$ , then there is another possibility: there is a stable alignment of the atomic magnets entailing this value  $I_w$  of the magnetization, which can maintain itself indefinitely if it should ever come into being. Do not leap to the other extreme, and suppose that this is a *perfect* alignment of the atomic magnets and hence a perfect saturation of the domain. Such a situation could exist (according to the theory) only at absolute zero. The equation gives us  $I_w$  as function of  $T$ , and this function declines smoothly from the value  $N\mu$  (for a domain of unit volume!) at absolute zero, to the value zero at  $T = \theta$ . The curve between these two points is completely determined by the values of  $\mu$  and  $\theta$ , which are derived in such ways as I have indicated from the magnetic properties of the substance at the higher temperatures well above  $\theta$ .

And now, the culmination. The so-called saturation of iron—the ordinate of the  $I$ -vs- $H$  curve when it flattens out and becomes sensibly parallel to the axis of abscissae—is itself (as I mentioned) a function of temperature; *it is this same function* (Fig. 3). What is usually called “saturation” with ferromagnetic bodies consists in aligning the big arrows of the domains, so that in unison of direction they exhibit that value of magnetization which is dictated by their internal temperature and internal field. “True” saturation—“saturation of saturations”—the alignment of the atoms within each domain superposed on the alignment of the domains with the field—this can be attained only at the absolute zero of temperature. We are able, however, to work at temperatures so close to absolute zero, that the remaining degree of extrapolation is slight; and we are able, therefore, to give with much

confidence values for the true saturation of iron, nickel, cobalt, gadolinium, and many ferromagnetic alloys.

(The reader may properly wonder why, instead of solving equation (12) obtained by putting  $H = 0$  in equation (10), it is not the practice to put for  $H$  the field strengths actually applied to iron when aligning

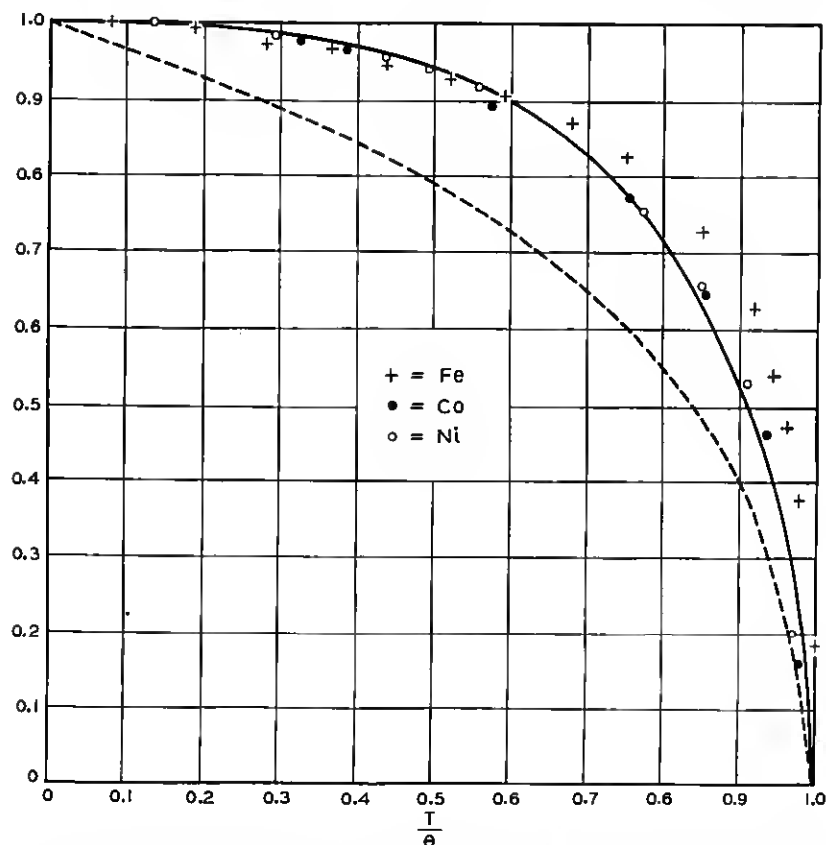


Fig. 3—Intrinsic magnetization plotted against  $T/\theta$ , for the domains of three ferromagnetic elementary metals (the constant  $\theta$  has different values for the three). The ordinate is  $I_0$  referred to its saturation-value (deduced by extrapolation) as unity. The curves are theoretical, the dashed one by classical theory ( $n = \infty$ ), the full one by quantum-theory ( $n = 2$ ).

the domains or in any other circumstances, and to solve the equation (10) under these conditions? This of course is the correct procedure, but in ferromagnetic bodies  $AI$  is usually so enormous by comparison with  $H$ , that the latter may be disregarded without appreciable error.)

One naturally asks about the size and the magnetic moment of the domains. It is useless to remember how the latter was determined for paramagnetic bodies from the features of their  $I$ -vs- $H$  curves, since the theory which made that possible is not applicable here. Moreover, the super-atoms share with ordinary atoms the quality of being invisible; no feature of the ordinary surface of a metal indicates them, and no technique of etching the surface seems able to delineate them. (It must be said, however, that ferromagnetic powders sprinkled over ferromagnetic metals may distribute themselves in remarkable picturesque patterns, and perhaps these sometimes simulate the pattern of the underlying domains.\*) But fortunately the super-atoms are not inaudible; at least, it is not a very extravagant statement to say that they can be heard. Let a girdle of wire around a rod of some ferromagnetic substance be connected through an amplifier with a microphone, and let a gradually-increasing magnetic field act lengthwise on the rod: the microphone will then emit a machine-gun patter of sharp clicks (with suitable amplification it may be very dramatic!) each of which corresponds to the sudden shift of the magnetic moment or "big arrow" of a domain from one of its possible orientations to another. Now if an electrical instead of an acoustical device is attached to the girdle of wire, the magnitude of the moment which thus re-orient itself at a single click may be assessed. It turns out that the moments are of very various magnitudes; a mean may, however, be estimated, and this mean is some  $10^{15}$  times as great as the moment of a single atom. Therefore the average domain comprises a million billions of atoms, and must therefore be about .002 cm in breadth; but there is a wide range of sizes about the average. As for the individual atoms of the ferromagnetic metals, their moments may be derived from equating  $N\mu$  to the values (obtained by extrapolation from observations at various low temperatures, to absolute zero) of that "saturation of saturations" defined above. They are by no means out of the common. Iron and its congeners are readily magnetizable, not because their atoms are extraordinarily magnetic—which is not at all the case—but because their atoms have this curious propensity of cohering together in large groups, developed to an extraordinary degree.

To many features of ferromagnetism, of which whole monographs might be or have been written, I can give only brief mention or none at all. There are the "magneto-caloric effects," arising because, when a ferromagnetic body is heated, the dis-alignment of the atoms

\* Cf. the article of R. M. Bozorth in the preceding number (January 1936) of this Journal.

within each domain increases, and this increase requires additional heat over and above that which goes to augment the kinetic energy of the atoms. The specific heat of iron (as of its congeners) is greater than it would be, but for this effect; the excess may be computed from the foregoing theory as function of temperature, and the computed values agree with the data to an extent which speaks very strongly for the theory. (The like is the case with a paramagnetic body exposed to a magnetic field; and as a result, such a body will grow cooler when the applied field is withdrawn, the kinetic energy of the atoms being levied upon when the dis-alignment occurs. The effect is imperceptible in usual circumstances, but with such substances as iron-ammonium alum at liquid-helium temperatures, it becomes so strong that the lowest temperatures ever achieved have been attained by making use of it.) There are the "magnetostrictive effects," arising because, when the atoms of the domains change their orientation, the metal as a whole is strained. It follows that there are interrelations between magnetization, strain, and stress; and anyone remembering even a little of the mathematical theory of elasticity with its moduli and its stress-strain tensors will readily believe that the theory of these interrelations is marvelously complicated. As one sensational example of the consequences, I cite the fact that when a certain permalloy is exposed to a field of, say, one half of one gauss, its magnetization ranges between a few per cent and nearly one hundred per cent of saturation, according to the strength of the tensile stress applied to it. The many processes of the metallurgical arts have often vast effects upon the magnetic properties of the ferromagnetic metals exposed to them: some are due to the changes in the elasticity and hence in the magnetostrictive effects, some to the changes in the chemical constitution (e.g. in the proportion of impurities), some to the changes in phase (of alloys) which these processes entail; but it would be risky to affirm that they have all been traced to one or another of these causes. The finer details in the shape of the  $I$ -vs- $H$  curve for ferromagnetics remain to be explained, and to account for one of them it seems to be thought necessary to assume that the domains may gain or lose in size at one another's expense; it is too bad that this impairs the concept of the domain as an immutable super-atom. I leave without overmuch regret this infinitely detailed and complicated topic, to conclude by brief allusions to the spinning electron and to diamagnetism.

Hitherto in these pages I have let it be inferred that when we obtain the magnetic moment of the atom of some element or the molecules of some compound by magnetic experiments upon the substance, it

always agrees with the "theoretical" value derived from the spectrum of that substance when a gas. This is indeed the case with gases and even with a certain number of solids, a large enough number to inspire confidence in the theory. There are, however, numerous exceptions among solids—a circumstance not to be wondered at, since an atom incorporated in a solid is usually in a very different condition from an atom freely wandering about in a gas. The like is true about that number  $n$ , the "number of permitted orientations of the atom in a field," which was introduced near the beginning of the article. Either the trend of the  $I$ -vs- $H$  curve for a paramagnetic, or the trend of the  $I_w$ -vs- $H$  curve for a ferromagnetic, enables us (if it has been sufficiently well measured) to ascertain the value of  $n$ ; and in a surprising number of instances, comprising iron, cobalt and nickel as well as various rare-earth elements in chemical compounds, the curves prescribe the value *two*, when the free atom according to its spectrum would display some other value. Thus when the atoms are compacted together into a solid, their proximity affects them in such a way as to bring about this result.

Now the important point about this value *two* for  $n$  is, that it is the value to be expected for an electron which is either isolated, or else linked to its atom in such a way that it has no orbital angular momentum. The contemporary theory of spectra includes, as one of its essential elements, the postulate of the "spinning electron"—the postulate that each electron by itself is endowed with an intrinsic and indestructible angular momentum and magnetic moment, of definite known amounts, having nothing whatever to do with its orbital revolutions.<sup>4</sup> This angular momentum or "electron-spin" is of the amount which requires  $n = 2$ , when it is not compounded with an angular momentum of orbital motion or with angular momenta of other electrons. The atoms in question behave, when compacted into solids, as though this angular momentum of individual spinning electrons were the only one left outstanding.

This striking inference is greatly strengthened by measurements upon the one phenomenon in which that angular momentum, which according to atomic theory is always the companion of magnetic moment, comes to light. Imagine a cylinder of some paramagnetic or ferromagnetic substance, hanging freely from a suspension attached to one end. Suppose it to be unmagnetized at first; this signifies that the atoms (whether or not they are grouped into domains) are so oriented that the resultant of all their angular momenta, as well as

<sup>4</sup> The reasons furnished by spectroscopy for making this postulate are much too complex to be interpolated in this article: I refer to the first fourteen pages of "Contemporary Advances in Physics," XXIX, this *Journal*, 14, 285-321 (April 1935).

that of all their magnetic moments, are zero. Now suppose that a field is suddenly applied, parallel to the axis of the cylinder. The substance is suddenly magnetized; this signifies that the resultant of the magnetic moments, and hence that of the angular momenta, are no longer zero. Let  $I$  stand (as heretofore) for the former resultant, and  $P$  for the latter. Now it is desirable to remember that each atom consists of a nucleus and an electron-family; that the electron-family possesses the magnetic moment and is oriented in the field (it strictly is what I have hitherto referred to as "the atom"); that the nuclei of the atoms in the cylinder are relatively non-magnetic but contain nearly all of the mass of the cylinder.<sup>5</sup> At the moment of magnetization, the ensemble of the electron-families acquires a net angular momentum  $P$ . Now angular momentum being one of these things (like energy and linear momentum) of which the total in Nature does not vary, an equal and opposite amount,  $-P$ , must appear somewhere or other. It appears in the mass of the cylinder, presumably because of some interaction between the electron-families and their nuclei. The cylinder makes a sharp turn at the instant of magnetization, twisting the suspension from which it hangs through an angle from which (and from the rigidity of the suspension) the value of  $-P$  can be found. This effect and its converse (an unmagnetized cylinder may be magnetized by sharply twisting it) are known as the "gyromagnetic effects." They are delicate and difficult to produce, *a fortiori* to measure; yet of late years experimenters have succeeded in measuring  $P$  together with  $I$ , and therefore learning the value of the ratio  $I/P$ —first for the ferromagnetic metals and then for some of their compounds and alloys, and lately for certain paramagnetic salts, the work on these last being done at the very low temperatures where alone they can be strongly magnetized.

This ratio  $I/P$ —its reciprocal is called the "gyromagnetic ratio"—is a rare sort of thing: it is a quantity of which the numerical value, measured on pieces of bulk matter, is appropriate also to the elementary particles. If the substance is made up of identical elementary magnets of magnetic moment  $\mu$  and angular momentum  $p$ , then  $I/P$  is  $\mu/p$ . Since  $\mu$  and  $p$  are knowable from spectra, so also is their ratio. Its lowest possible value (from theory) is  $e/2mc$ , in which  $e$ ,  $m$ , and  $c$  have their usual meanings;<sup>6</sup> this would always occur if the electrons had no spins; actually it occurs if the electron-family of the atom is so

<sup>5</sup> Most nuclei possess magnetic moments, which, however, are so excessively small that they can be detected only by experiments of extreme delicacy.

<sup>6</sup> Charge (in E.S.U.) and mass of the electron, and speed of light in vacuo. For the theory underlying these statements, c.f. *l.c.* pp. 285–300. Often the ratio of the experimental value of  $\mu/p$  to the quantity  $e/2mc$  is called an "experimental  $g$ -value," the ratio of the theoretical value to  $e/2mc$  being conventionally denoted by  $g$ .

organized that the spins neutralize one another. Its highest possible value is twice as great; this occurs if nothing counts excepting the electron-spins, and signifies either that the electrons are free<sup>7</sup> or else that the electron-family of each atom is so organized that there is no net angular momentum due to orbital motion. Intermediate values are possible and signify different types of organization of the electron-family. The values predicted from spectra have been confirmed for a few of the rare-earth atoms in their paramagnetic salts; but usually, as I have already intimated, the *observed value of the ratio  $\mu/p$*  is about  $2(e/2mc)$ , though the spectrum says something else.

It would be pleasant now to add that the magnetic moment of each of these substances, per atom, amounts to some integer multiple of the magnetic moment  $\mu_e$  of the spinning electron. We then could say that the integer is the number of "uncompensated" spinning electrons in the atom, implying by the word "uncompensated" in this connection that all the magnetic moments in the electron-family of the atom add up vectorially to zero and so do all the angular momenta, with the sole exception of those pertaining to these electron-spins. Such is not, however, the case: some of the experimental values are  $2.2\mu_e$  for iron,  $1.7\mu_e$  for cobalt,  $0.6\mu_e$  for nickel. It seems necessary to assume that in metallic solid iron, some of the atoms present two uncompensated electrons to the orienting field, and others three. Iron in different chemical compounds exhibits different values of magnetic moment, and sometimes the ratio  $\mu/p$  is different from  $2(e/2mc)$ , suggesting that angular momenta of orbital motion are not quite cancelled out; indeed it now appears that the ratio is slightly but definitely different from this specific value even in the cases (such as those of the pure ferromagnetic metals and of permalloy) in which at first the measurements suggested that it was the same.

Such observations as these last are problems for the specialists in atomic theory; magnetism offers great numbers of these problems. Another and a complementary way of viewing this situation is, to look on every measurement of a magnetic moment made upon a solid as an item of information about an atom (or a molecule) existing in a condition which is not accessible to spectroscopic research. Spectra indicate the normal state of atoms in freedom; occasional magnetic experiments (like those on gaseous oxygen here cited, or those on molecular beams by the Gerlach-Stern method, which I hope to treat on a later occasion) also refer to free atoms and molecules, and confirm the indications of the spectra, thus sustaining both the methods; but

<sup>7</sup> Certain metals, the alkali metals for instance, exhibit a paramagnetism which is entirely due to the "free" or conduction electrons.

mostly the magnetic methods refer to atoms in a solid, and so they make available a new and broad domain for the operations of atomic theory.

There remains diamagnetism. The first thing to be said about the theory of diamagnetism is discouraging; for it has the earmark of a futile atomic theory—it involves the assumption that the individual atoms behave exactly like the substance as a whole. Under all field strengths and all conditions, it is assumed that the diamagnetic moment of a block of  $N$  atoms is  $N$  times the diamagnetic moment of a single atom. Nevertheless this is not a futile assumption, for strictly it is not an assumption at all but an inference from atomic structure. It was mentioned early in these pages that owing to the unbreakable link between angular momentum and magnetic moment, a magnetic atom precesses about the direction of the field. This motion of precession is an extra motion of the electrons of the atoms, a circulatory motion around the axis supplied by the direction of the field. This extra motion entails an extra current, which entails an extra magnetic moment, which is the source of diamagnetism or which *is* diamagnetism. *Diamagnetism is precession.* It is not confined, as the foregoing words suggest, to atoms which have a net magnetic moment. Consider an atom (a free atom of any noble gas will afford an example) possessing two or more electrons, the orbits and the spins of which are so oriented that the resultant magnetic moment is nil. Though some of the orbits and spins are pointed oppositely to others, they all precess in the same sense, and the atom acquires a magnetic moment in the field though it had none beforehand. The like is true, of course, when the resultant of the orbits and the spins is different from zero; the agents of orientation which were discussed above render it paramagnetic, but the precession renders it diamagnetic, and it is paramagnetic and diamagnetic—or ferromagnetic and diamagnetic—at one and the same time. The moment due to the precession is proportional to the field strength, and the factor of proportionality may be calculated from the structure of the atom (it depends primarily upon the areas of the electron-orbits). The agreement of the calculated values with the data is generally satisfactory; and diamagnetism, the least conspicuous of the three types of magnetism, takes precedence over the others as being that one of the three of which our understanding is most nearly perfect.